Conclusions and Implications

NGSS will require major shifts in science education (NRC, 2011; see Table 1), comparable to major shifts due to CCSS for English language arts and literacy and for mathematics (Common Core State Standards Initiative, 2010a, 2010b). Across these three subject areas, the new standards share a common emphasis on disciplinary practices and classroom discourse (see Figure 1). As engagement in these practices is language intensive, it presents both language demands and opportunities for all students, especially ELLs.

Given the richness of science and engineering practices, NGSS will lead to science classrooms that are also rich language learning environments for ELLs. An important role of the science teacher is to encourage and support language use and development in the service of making sense of science. Participation in science and engineering practices should be expected of all students, and ELLs' contributions should be accepted and acknowledged for their value within the science discourse, rather than critiqued for their "flawed" use of language. This view is consistent with contemporary literature on language in science learning and teaching that highlights what students and teachers *do* with language as they engage in science inquiry and discourse practices (Carlsen, 2007; G. Kelly, 2007). This view is also consistent with current theories of SLA that emphasize what learners *can do* with language—the socially oriented (rather than individually oriented) view of SLA and the experiential (rather than structural) pedagogies.

Although content-based language instruction, sheltered instruction, and academic language instruction are valuable attempts to bring together subject matter instruction and second language instruction, their predominant emphases have been on the study and practice of language elements rather than on immersion in rich environments that use language for sense-making. In this article, we indicate how science and engineering practices involve a range of analytical tasks and language functions (see Table 2). We also stress the value of attention to the *language of the science classroom* that moves toward the disciplinary language of science (see Table 3).

NGSS will present both the need and opportunity to address a new set of research questions. What do science and engineering practices look like in the science classroom? How does student ability to engage in these practices progress over the grade levels? What are the supports needed for such engagement at a given grade level? What do science teachers and language specialists need to know about language demands and opportunities to support ELLs' engagement in these practices? What additional supports, both within and outside the science classroom, do ELLs need in order to most rapidly gain content-relevant language skills? What technologies and tools most effectively support this language learning?

Successful implementation of NGSS with ELLs will require political will, especially in the current accountability policy context, where these students tend to receive limited and inequitable science instruction because of the perceived urgency of developing basic literacy and numeracy. To the contrary, we argue that NGSS can provide a context where science learning and language learning can occur simultaneously. We also argue that ELLs' success in the science classroom will depend on shared responsibilities of teachers across subject areas, as learning of science and development of literacy and numeracy reinforce one another.

NOTES

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We appreciate Tina Cheuk for developing Figure 1. We also appreciate Kenji Hakuta at Stanford University, Andrés Henríquez at Carnegie Corporation of New York, and Cory Buxton at the University of Georgia for their valuable feedback to the draft versions of the manuscript.

We would like to highlight that the article is based on collaboration among a science educator (Okhee Lee), a scientist (Helen Quinn), and a second language acquisition educator (Guadalupe Valdés), which represents the kinds of collaboration advocated in this article. We hope collaboration of this nature will become more common and frequent as CCSS and NGSS are implemented with all students, including ELLs.

1. In a companion paper that is posted on the project website ell.stanford.edu, we have offered effective classroom strategies to support science and language for ELLs in five domains: (a) literacy strategies for all students, (b) language support strategies with ELLs, (c) discourse strategies with ELLs, (d) home language support, and (e) home culture connections (Quinn, Lee, & Valdés, 2012).

2. We put the term "flawed" in quotations to stress that we do not consider incorrect grammar or nonnative-like speech to be a problem to be corrected in the moment. Rather, we stress that contributions should be valued for their role in the discourse regardless of the language level of the speaker.

3. The term *register* has been used in sociolinguistics to refer to a style of language defined by its context and different occasions of use. Ferguson (1994), one of the first scholars to use the term, defined register variation as ways of speaking in regularly occurring communication situations that, over time, will tend to develop similar vocabularies, characteristics of intonation and structure, and bits of syntax, that are different from the language of other communication situations. For a foundational discussion of register in sociolinguistics, the reader is referred to Biber and Finegan (1994). It is noted that Halliday and colleagues have a highly specialized definition of register that is not broadly used in general sociolinguistics. In this article, we use the term "register" following Ferguson and also "ways and styles of speaking" to refer to these regularly occurring uses of language.

4. For example, Solomon and Rhodes (1995) point out that academic language has generally been described in discrete linguistic terms focusing on lexis and syntax. It has also been described as "a compilation of unique language functions and structures that is difficult for minority students to master" (p. 2). Valdés (2004) lists various definitions of academic language used in the literature including "a set of intellectual practices," "language that follows stylistic conventions and is error free," "the language used in particular disciplines," and "the language needed to succeed academically in all content areas" (pp. 17–19).

5. Both students and teachers use "regular ways of speaking" for different purposes in the classroom. These different ways of speaking (e.g., talk between classmates, talk before the whole class, talk to present formal classroom reports) are acquired by individuals in the course of regularly interacting for particular purposes and in particular contexts. As pointed out in Footnote 3, a register is an accepted and shared "way of speaking" for specific purposes by individuals in interaction.

REFERENCES

Bachman, L. F. (1990). *Fundamental considerations in language testing*. New York, NY: Oxford University Press.

Biber, D., & Finegan, E. (Eds.). (1994). *Sociolinguistic perspectives on register*. Oxford, UK: Oxford University Press.

Brinton, D. M., Snow, M. A., & Wesche, M. B. (1989). *Content-based second language instruction.* New York: Newbury House.

Brown, B. A. (2006). "It isn't slang that can be said about this stuff": Language, identity, and appropriating science discourse. *Journal of Research in Science Teaching, 43*, 96–126.

Brown, B. A., & Ryoo, K. (2008). Teaching science as a language: A "content-first" approach to science teaching. *Journal of Research in Science Teaching, 45*, 529–553.

Canale, M., & Swain, M. (1980). Theoretical bases of communicative approaches to second language teaching and testing. *Applied Linguistics, 1*, 1–47.

Carlsen, W. S. (2007). Language and science learning. In S. K. Abell & N. G. Lederman (Eds.), *Handbook of research on science education* (pp. 57–74). Mahwah, NJ: Lawrence Erlbaum.

Common Core State Standards Initiative. (2010a). *Common Core State Standards for English language arts and literacy in history/social studies, science, and technical subjects.* Retrieved from http://www.corestandards.org

Common Core State Standards Initiative. (2010b). *Common Core State Standards for mathematics.* Retrieved from http://www.corestandards.org

Cornelius, L. L., & Herrenkohl, L. R. (2004). Power in the classroom: How the classroom environment shapes students' relationships with each other and with concepts. *Cognition and Instruction, 22*, 467–498.

Council of Chief State School Officers. (2012). *Framework for English language proficiency development standards corresponding to the Common Core State Standards and the Next Generation Science Standards*. Washington, DC: Author.

Coxhead, A. (1998). *An academic word list.* Wellington, New Zealand: Victoria University of Wellington, School of Linguistics and Applied Language Studies.

Duff, P. A. (1995). An ethnography of communication in immersion classrooms in Hungary. *TESOL Quarterly, 29*, 505–537.

Duff, P. A. (2002). The discursive co-construction of knowledge, identity, and difference: An ethnography of communication in the high school mainstream. *Applied Linguistics, 23*, 289–322.

Echevarria, J., & Short, D. (2006). School reform and standards-based education: A model for English language learners. *Journal of Educational Research, 99*, 195–211.

Echevarria, J., & Vogt, M. E. (2008). *Making content comprehensible for English learners.* Boston, MA: Allyn & Bacon.

Ellis, R. (2005). *Instructed second language acquisition: A literature review.* Wellington, Australia: Research Division, Ministry of Education.

Engle, R. A., & Conant, F. (2002). Guiding principles for fostering productive disciplinary engagement: Explaining an emergent argument in a community of learners' classroom. *Cognition and Instruction, 20*, 399–483.

Fang, Z., & Schleppegrell, M. (2008). *Reading in secondary content areas: A language-based pedagogy.* Ann Arbor, MI: University of Michigan Press.

Ferguson, C. (1994). Dialect, register, and genre: Working assumptions about conventionalization. In D. Biber & E. Finegan (Eds.), *Sociolinguistic perspectives on register* (pp. 15–30). Oxford, UK: Oxford University Press.

Gass, S. M., & Selinker, L. (2001). *Second language acquisition: An introductory course*. Mahwah, NJ: Lawrence Erlbaum.

Gee, J. P. (1990). *Social linguistics and literacies: Ideology in discourses. Critical perspectives on literacy and education*. London: Falmer Press.

Halliday, M. A. K. (2002). *On grammar in the collected works of M.A.K. Halliday* (Vol. 1). London: Continuum.

Halliday, M. A. K., & Martin, J. R. (1993). *Writing science: Literacy and discursive power.* London: The Falmer Press.

Halliday, M. A. K., & Matthiessen, C. M. (2004). *An introduction to functional grammar* (3rd ed.). London: Arnold.

Johnson, M. (2004). *A philosophy of second language acquisition*. New Haven, CT: Yale University Press.

Kelly, G. (2007). Discourse in science classrooms. In S. K. Abell & N. G. Lederman (Eds.), *Handbook of research on science education* (pp. 443–469). Mahwah, NJ: Lawrence Erlbaum.

Kelly, L. G. (1969). *Twenty-five centuries of language teaching.* Rowley, MA: Newbury House.

Lantolf, J. P. (Ed.). (2000). *Sociocultural theory and second language development.* Oxford, UK: Oxford University Press.

Lantolf, J. P. (2006). Sociocultural theory and L2. *Studies in Second Language Acquisition, 28*, 67–109.

Lee, O., Buxton, C. A., Lewis, S., & LeRoy, K. (2006). Science inquiry and student diversity: Enhanced abilities and continuing difficulties after an instructional intervention. *Journal of Research in Science Teaching, 43*, 607–636.

Lee, O., & Fradd, S. H. (1998). Science for all, including students from non-English language backgrounds. *Educational Researcher, 27*, 12–21.

Lemke, J. L. (1990). *Talking science: Language, learning and values.* Norwood, NJ: Ablex.

Long, M. H. (1983). Native speaker/non-native speaker conversation and the negotiation of comprehensible input. *Applied Linguistics, 4*, 126–141.

Long, M. (1991). Focus on form: A design feature in language teaching methodology. In K. de Bot, R. Ginsberg, & C. Kramsch (Eds.), Foreign language research in cross-cultural perspective (pp. 39–52). Amsterdam: John Benjamins.

Long, M. H. (1996). The role of the linguistic environment in second language acquisition. *Handbook of Second Language Acquisition, 26*, 413–468.

National Research Council. (2011). *A framework for K-12 science education: Practices, crosscutting concepts, and core ideas.* Washington, DC: National Academies Press.

Norris, J. M., & Ortega, L. (2000). Effectiveness of L2 instruction: A research synthesis and quantitative meta-analysis. *Language Learning, 50*, 417–528.

Norris, J. M., & Ortega, L. (2006). *Synthesizing research on language learning and teaching* (Vol. 13). Amsterdam: John Benjamins.

Pica, T. (2008). Task-based teaching and learning. In B. Spolsky & F. K. Hult (Eds.), *The handbook of educational linguistics* (pp. 525–538). Malden, MA: Wiley-Blackwell.

Quinn, H., Lee, O., & Valdés, G. (2012). *Language demands and opportunities in relation to Next Generation Science Standards for English language learners: What teachers need to know.* Stanford, CA: Stanford University, Understanding Language Initiative at Stanford University (ell.stanford.edu).

Rosebery, A. S., Warren, B., & Conant, F. R. (1992). Appropriating scientific discourse: Findings from language minority classrooms. *The Journal of the Learning Sciences, 21*, 61–94.

Scarcella, R. (2003). *Academic English: A conceptual framework* (Technical Report No. 2003-1, No. 1). Santa Barbara, CA: The University of California Linguistic Minority Research Institute.

Schleppegrell, M. J. (2004). *The language of schooling: A functional linguistic perspective*. Mahwah, NJ: Lawrence Erlbaum.

Snow, M. A. (2001). Content-based and immersion models for second and foreign language teaching. In M. Celce-Murcia (Ed.), *Teaching English as a second or foreign language* (3rd ed., pp. 303–318). Boston, MA: Heinle & Heinle.

Solomon, J., & Rhodes, N. C. (1995). *Conceptualizing academic language* (Research Report No. 15). Washington, DC: National Center for Research on Cultural Diversity and Second Language Learning. (ED38912)

Stern, H. H. (1990). Analysis and experience as variables in second language pedagogy. In B. Harley, P. Allen, J. Cummins, & M. Swain (Eds.), The development of second language proficiency (pp. 93 –109). Cambridge, UK: Cambridge University Press.

Valdés, G. (2004). Between support and marginalisation: The development of academic language in linguistic minority children. *International Journal of Bilingual Education and Bilingualism*, *7*, 102–132.

Warren, B., Rosebery, A. S., & Conant, F. (1994). Discourse and social practice: Learning science in language minority classrooms. In D. Spencer (Ed.), *Adult biliteracy in the United States* (pp. 191–210). Washington, DC: Center for Applied Linguistics and Delta Systems Co.

Windschitl, M., Thompson, J., & Braaten, M. (2011). Fostering ambitious pedagogy in novice teachers: The new role of tool-supported analyses of student work. *Teachers College Record, 113*, 1311–1360.

Wong Fillmore, L. (1992). Learning a language from learners. In C. Kramsch & S. McConnel-Ginnet (Eds.), *Text and context: Cross-disciplinary perspectives on language study* (pp. 46–66). Lexington, MA: Heath.

Zuengler, J., & Miller, E. R. (2006). Cognitive and sociocultural perspectives: Two parallel SLA worlds. *TESOL Quarterly, 40*, 35–58.

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On Proof and Progress in Mathematics [1]

WILLIAM P. THURSTON

This essay on the nature of progress in mathematics was stimulated by the article of Jaffe and Quinn, "Theoretical Mathematics: Toward a cultural synthesis of mathematics and theoretical physics" [2] Their article raises interesting issues that mathematicians should pay more attention to, but it also perpetuates some widely held beliefs and attitudes that need to be questioned and examined

The article had one paragraph portraying some of my work in a way that diverges from my experience, and it also diverges from the observations of people in the field whom I've discussed it with as a reality check

After some reflection, it seemed to me that what Jaffe and Quinn wrote was an example of the phenomenon that people see what they are tuned to see. Their portrayal of my work resulted from projecting the sociology of mathematics onto a one-dimensional scale (speculation versus rigor) that ignores many basic phenomena.

Responses to the Jaffe-Quinn article have been invited from a number of mathematicians, and I expect it to receive plenty of specific analysis and criticism from others Therefore, I will concentrate in this essay on the positive rather than on the contranegative. I will describe my view of the process of mathematics, referring only occasionally to Jaffe and Quinn by way of comparison

In attempting to peel back layers of assumptions, it is important to try to begin with the right questions:

1. What is it that mathematicians accomplish?

There are many issues buried in this question, which I have tried to phrase in a way that does not presuppose the nature of the answer

It would not be good to start, for example, with the question

> How do mathematicians prove theorems?

This question introduces an interesting topic, but to start with it would be to project two hidden assumptions:

(1) that there is uniform, objective and firmly established theory and practice of mathematical proof, and

(2) that progress made by mathematicians consists of proving theorems

It is worthwhile to examine these hypotheses, rather than to accept them as obvious and proceed from there.

The question is not even

> How do mathematicians make progress in mathematics?

Rather, as a more explicit (and leading) form of the question, I prefer

> How do mathematicians advance human understanding of mathematics?

This question brings to the fore something that is fundamental and pervasive: that what we are doing is finding ways for *people* to understand and think about mathematics.

The rapid advance of computers has helped dramatize this point, because computers and people are very different For instance, when Appel and Haken completed a proof of the 4-color map theorem using a massive automatic computation, it evoked much controversy. I interpret the controversy as having little to do with doubt people had as to the veracity of the theorem or the correctness of the proof. Rather, it reflected a continuing desire for *human understanding* of a proof, in addition to knowledge that the theorem is true.

On a more everyday level, it is common for people first starting to grapple with computers to make large-scale computations of things they might have done on a smaller scale by hand. They might print out a table of the first 10,000 primes, only to find that their printout isn't something they really wanted after all. They discover by this kind of experience that what they really want is usually not some collection of "answers"—what they want is *understanding*.

It may sound almost circular to say that what mathematicians are accomplishing is to advance human understanding of mathematics. I will not try to resolve this by discussing what mathematics is, because it would take us far afield Mathematicians generally feel that they know what mathematics is, but find it difficult to give a good direct definition. It is interesting to try. For me, "the theory of formal patterns" has come the closest, but to discuss this would be a whole essay in itself.

Could the difficulty in giving a good direct definition of mathematics be an essential one, indicating that mathematics is the smallest subject satisfying the following:

- Mathematics includes the natural numbers and plane and solid geometry.
- Mathematics is that which mathematicians study.
- Mathematicians are those humans who advance human understanding of mathematics.

In other words, as mathematics advances, we incorporate it into our thinking As our thinking becomes more sophisticated, we generate new mathematical concepts and new mathematical structures: the subject matter of mathematics changes to reflect how we think

If what we are doing is constructing better ways of thinking, then psychological and social dimensions are essential to a good model for mathematical progress These dimensions are absent from the popular model In caricature, the popular model holds that

D. mathematicians start from a few basic mathematical structures and a collection of axioms "given" about these structures, that

T there are various important questions to be answered about these structures that can be stated as formal mathematical propositions, and

P the task of the mathematician is to seek a deductive pathway from the axioms to the propositions or to their denials

We might call this the definition-theorem-proof (DTP) model of mathematics

A clear difficulty with the DTP model is that it doesn't explain the source of the questions Jaffe and Quinn discuss speculation (which they inappropriately label "theoretical mathematics") as an important additional ingredient Speculations consists of making conjectures, raising questions, and making intelligent guesses and heuristic arguments about what is probably true.

Jaffe and Quinn's DSTP model still fails to address some basic issues. We are not trying to meet some abstract production quota of definitions, theorems and proofs. The measure of our success is whether what we do enables *people* to understand and think more clearly and effectively about mathematics.

Therefore, we need to ask ourselves:

2. How do people understand mathematics?

This is a very hard question. Understanding is an individual and internal matter that is hard to be fully aware of, hard to understand and often hard to communicate. We can only touch on it lightly here.

People have very different ways of understanding particular pieces of mathematics. To illustrate this, it is best to take an example that practicing mathematicians understand in multiple ways, but that we see our students struggling with. The derivative of a function fits well. The derivative can be thought of as:

(1) Infinitesimal: the ratio of the infinitesimal change in the value of a function to the infinitesimal change in a function

(2) Symbolic: the derivative of x^n is nx^{n-1}, the derivative of $\sin(x)$ is $\cos(x)$, the derivative of $f \circ g$ is $f' \circ g * g'$, etc.

(3) Logical: $f'(x) = d$ if and only if for every ε there is a δ such that when $0 < |\Delta x| < \delta$,

$$\left| \frac{f(x+\Delta x) - f(x)}{\Delta x} - d \right| < \delta$$

(4) Geometric: the derivative is the slope of a line tangent to the graph of the function, if the graph has a tangent.

(5) Rate: the instantaneous speed of $f(t)$, when t is time

(6) Approximation: The derivative of a function is the best linear approximation to the function near a point

(7) Microscopic: The derivative of a function is the limit of what you get by looking at in under a microscope of higher and higher power.

This is a list of different ways of *thinking about* or *conceiving of* the derivative, rather than a list of different *logical definitions*. Unless great efforts are made to maintain the tone and flavor of the original human insights, the differences start to evaporate as soon as the mental concepts are translated into precise, formal and explicit definitions.

I can remember absorbing each of these concepts as something new and interesting, and spending a good deal of mental time and effort digesting and practicing with each, reconciling it with the others. I also remember coming back to revisit these different concepts later with added meaning and understanding.

The list continues; there is no reason for it ever to stop. A sample entry further down the list may help illustrate this. We may think we know all there is to say about a certain subject, but new insights are around the corner. Furthermore, one person's clear mental image is another person's intimidation:

37. The derivative of a real-valued function f in a domain D is the Lagrangian section of the cotangent bundle $T^*(D)$ that gives the connection form for the unique flat connection on the trivial $\mathbf{R}$-bundle $D \times \mathbf{R}$ for which the graph of f is parallel.

These differences are not just a curiosity. Human thinking and understanding do not work on a single track, like a computer with a single central processing unit Our brains and minds seem to be organized into a variety of separate, powerful facilities. These facilities work together loosely, "talking" to each other at high levels rather than at low levels of organization.

Here are some major divisions that are important for mathematical thinking:

(1) Human language. We have powerful special-purpose facilities for speaking and understanding human language, which also tie in to reading and writing Our linguistic facility is an important tool for thinking, not just for communication. A crude example is the quadratic formula which people may remember as a little chant, "ex equals minus bee plus or minus the square root of bee squared minus four ay see all over two ay " The mathematical language of symbols is closely tied to our human language facility The fragment of mathematical symbolese available to most calculus students has only one verb, "=". That's why students use it when they're in need of a verb. Almost anyone who has taught calculus in the U.S. has seen students instinctively write "$x^3 = 3x^2$" and the like

(2) Vision, spatial sense, kinesthetic (motion) sense People have very powerful facilities for taking in information visually or kinesthetically, and thinking with their spatial sense. On the other hand, they do not have a very good built-in facility for inverse vision, that is, turning an internal spatial understanding back into a two-dimensional image Con-

sequently, mathematicians usually have fewer and poorer figures in their papers and books than in their heads

An interesting phenomenon in spatial thinking is that scale makes a big difference. We can think about little objects in our hands, or we can think of bigger human-sized structures that we scan, or we can think of spatial structures that encompass us and that we move around in. We tend to think more effectively with spatial imagery on a larger scale: it's as if our brains take larger things more seriously and can devote more resources to them.

(3) Logic and deduction We have some built-in ways of reasoning and putting things together associated with how we make logical deductions: cause and effect (related to implication), contradiction or negation, *etc.*

Mathematicians apparently don't generally rely on the formal rules of deduction as they are thinking. Rather, they hold a fair bit of logical structure of a proof in their heads, breaking proofs into intermediate results so that they don't have to hold too much logic at once. In fact, it is common for excellent mathematicians not even to know the standard formal usage of quantifiers (for all and there exists), yet all mathematicians certainly perform the reasoning that they encode

It's interesting that although "or", "and" and "implies" have identical formal usage, we think of "or" and "and" as conjunctions and "implies" as a verb.

(4) Intuition, association, metaphor. People have amazing facilities for sensing something without knowing where it comes from (intuition); for sensing that some phenomenon or situation or object is like something else (association); and for building and testing connections and comparisons, holding two things in mind at the same time (metaphor). These facilities "listening" to my intuitions and associations, and building them into metaphors and connections. This involves a kind of simultaneous quieting and focusing of my mind. Words, logic, and detailed pictures rattling around can inhibit intuitions and associations

(5) Stimulus-response This is often emphasized in schools; for instance, if you see 3927×253, you write one number above the other and draw a line underneath, *etc.* This is also important for research mathematics: seeing a diagram of a knot, I might write down a presentation for the fundamental group of its complement by a procedure that is similar in feel to the multiplication algorithm.

(6) Process and time. We have a facility for thinking about processes or sequences of actions that can often be used to good effect in mathematical reasoning One way to think of a function is an action, a process, that takes the domain to the range. This is particularly valuable when composing functions. Another use of this facility is in remembering proofs: people often remember a proof as a process consisting of several steps. In topology, the notion of a homotopy is most often thought of as a process taking time. Mathematically, time is no different from one more spatial dimension, but since humans interact with it in a quite different way, it is psychologically very different.

3. How is mathematical understanding communicated?

The transfer of understanding from one person to another is not automatic. It is hard and tricky. Therefore, to analyze human understanding of mathematics, it is important to consider **who** understands **what**, and **when**.

Mathematicians have developed habits of communication that are often dysfunctional. Organizers of colloquium talks everywhere exhort speakers to explain things in elementary terms. Nonetheless, most of the audience at an average colloquium talk gets little of value from it. Perhaps they are lost within the first 5 minutes, yet sit silently through the remaining 55 minutes. Or perhaps they quickly lose interest because the speaker plunges into technical details without presenting any reason to investigate them At the end of the talk, the few mathematicians who are close to the field of the speaker ask a question or two to avoid embarrassment.

This pattern is similar to what often holds in classrooms, where we go through the motions of saying for the record what we think the students "ought" to learn, while the students are trying to grapple with the more fundamental issues of learning our language and guessing at our mental models Books compensate by giving samples of how to solve every type of homework problem Professors compensate by giving homework and tests that are much easier than the material "covered" in the course, and then grading the homework and tests on a scale that requires little understanding. We assume that the problem is with the students rather than with communication: that the students either just don't have what it takes, or else just don't care.

Outsiders are amazed at this phenomenon, but within the mathematical community, we dismiss it with shrugs

Much of the difficulty has to do with the language and culture of mathematics, which is divided into subfields. Basic concepts used every day within one subfield are often foreign to another subfield. Mathematicians give up on trying to understand the basic concepts even from neighboring subfields, unless they were clued in as graduate students.

In contrast, communication works very well within the subfields of mathematics Within a subfield, people develop a body of common knowledge and known techniques By informal contact, people learn to understand and copy each other's ways of thinking, so that ideas can be explained clearly and easily.

Mathematical knowledge can be transmitted amazingly fast within a subfield When a significant theorem is proved, it often (but not always) happens that the solution can be communicated in a matter of minutes from one person to another within the subfield. The same proof would be communicated and generally understood in an hour talk to members of the subfield It would be the subject of a 15- or 20-page paper, which could be read and understood in a few hours or perhaps days by members of the subfield

Why is there such a big expansion from the informal discussion to the talk to the paper? One-on-one, people use wide channels of communication that go far beyond formal mathematical language They use gestures, they draw pictures and diagrams, they make sound effects and use body language Communication is more likely to be two-way, so that people can concentrate on what needs the most attention. With these channels of communication, they are in a much better position to convey what's going on, not just in their logical and linguistic facilities, but in their other mental facilities as well.

In talks, people are more inhibited and more formal Mathematical audiences are often not very good at asking the questions that are on most people's minds, and speakers often have an unrealistic preset outline that inhibits them from addressing questions even when they are asked

In papers, people are still more formal. Writers translate their ideas into symbols and logic, and readers try to translate back.

Why is there such a discrepancy between communication within a subfield and communication outside of subfields, not to mention communication outside mathematics?

Mathematics in some sense has a common language: a language of symbols, technical definitions, computations, and logic. This language efficiently conveys some, but not all, modes of mathematical thinking Mathematicians learn to translate certain things almost unconsciously from one mental mode to the other, so that some statements quickly become clear. Different mathematicians study papers in different ways, but when I read a mathematical paper in a field in which I'm conversant, I concentrate on the thoughts that are between the lines. I might look over several paragraphs or strings of equations and think to myself "Oh yeah, they're putting in enough rigmarole to carry such-and-such idea." When the idea is clear, the formal setup is usually unnecessary and redundant—I often feel that I could write it out myself more easily than figuring out what the authors actually wrote. It's like a new toaster that comes with a 16-page manual. If you already understand toasters and if the toaster looks like previous toasters you've encountered, you might just plug it in and see if it works, rather than first reading all the details in the manual.

People familiar with ways of doing things in a subfield recognize various patterns of statements or formulas as idioms or circumlocution for certain concepts or mental images. But to people not already familiar with what's going on the same patterns are not very illuminating; they are often even misleading. The language is not alive except to those who use it.

I'd like to make an important remark here: there are some mathematicians who are conversant with the ways of thinking in more than one subfield, sometimes in quite a number of subfields. Some mathematicians learn the jargon of several subfields as graduate students, some people are just quick at picking up foreign mathematical language and culture, and some people are in mathematical centers where they are exposed to many subfields People who are comfortable in more than one subfield can often have a very positive influence, serving as bridges, and helping different groups of mathematicians learn from each other. But people knowledgeable in multiple fields can also have a negative effect, by intimidating others, and by helping to validate and maintain the whole system of generally poor communication. For example, one effect often takes place during colloquium talks, where one or two widely knowledgeable people sitting in the front row may serve as the speaker's mental guide to the audience.

There is another effect caused by the big differences between how we think about mathematics and how we write it. A group of mathematicians interacting with each other can keep a collection of mathematical ideas alive for a period of years, even though the recorded version of their mathematical work differs from their actual thinking, having much greater emphasis on language, symbols, logic and formalism. But as new batches of mathematicians learn about the subject they tend to interpret what they read and hear more literally, so that the more easily recorded and communicated formalism and machinery tend to gradually take over from other modes of thinking.

There are two counters to this trend, so that mathematics does not become entirely mired down in formalism. First, younger generations of mathematicians are continually discovering and rediscovering insights on their own, thus reinjecting a diverse modes of human thought into mathematics

Second, mathematicians sometimes invent names and hit on unifying definitions that replace technical circumlocutions and give good handles for insights. Names like "group" to replace "a system of substitutions satisfying. ", and "manifold" to replace

> We can't give coordinates to parametrize all the solutions to our equations simultaneously, but in the neighborhood of any particular solution we can introduce coordinates
>
> $(f_1(u_1, u_2, u_3), f_2(u_1, u_2, u_3), f_3(u_1, u_2, u_3), f_4(u_1, u_2, u_3), f_5(u_1, u_2, u_3))$
>
> where at least one of the ten determinants
>
> ...[ten 3×3 determinants of matrices of partial derivatives]...
>
> is not zero

may or may not have represented advances in insight among experts, but they greatly facilitate the *communication* of insights.

We mathematicians need to put far greater effort into communicating mathematical *ideas*. To accomplish this, we need to pay much more attention to communicating not just our definitions, theorems, and proofs, but also our ways of thinking. We need to appreciate the value of different ways of thinking about the same mathematical structure.

We need to focus far more energy on understanding and explaining the basic mental infrastructure of mathematics—with consequently less energy on the most recent results This entails developing mathematical language that is effective for the radical purpose of conveying ideas to people who don't already know them

Part of this communication is through proofs

4. What is a proof?

When I started as a graduate student at Berkeley, I had trouble imagining how I could "prove" a new and interesting mathematical theorem I didn't really understand what a "proof" was

By going to seminars, reading papers, and talking to other graduate students, I gradually began to catch on Within any field, there are certain theorems and certain techniques that are generally known and generally accepted. When you write a paper, you refer to these without proof. You look at other papers in the field, and you see what facts they quote without proof, and what they cite in their bibliography You learn from other people some idea of the proofs Then you're free to quote the same theorem and cite the same citations. You don't necessarily have to read the full papers or books that are in your bibliography. Many of the things that are generally known are things for which there may be no known written source. As long as people in the field are comfortable that the idea works, it doesn't need to have a formal written source

At first I was highly suspicious of this process. I would doubt whether a certain idea was really established But I found that I could ask people, and they could produce explanations and proofs, or else refer me to other people or to written sources that would give explanations and proofs. There were published theorems that were generally known to be false, or where the proofs were generally known to be incomplete. Mathematical knowledge and understanding were embedded in the minds and in the social fabric of the community of people thinking about a particular topic This knowledge was supported by written documents, but the written documents were not really primary

I think this pattern varies quite a bit from field to field I was interested in geometric areas of mathematics, where it is often pretty hard to have a document that reflects well the way people actually think. In more algebraic or symbolic fields, this is not necessarily so, and I have the impression that in some areas documents are much closer to carrying the life of the field But in any field, there is a strong social standard of validity and truth Andrew Wiles's proof of Fermat's Last Theorem is a good illustration of this, in a field which is very algebraic. The experts quickly came to believe that his proof was basically correct on the basis of high-level ideas, long before details could be checked. This proof will receive a great deal of scrutiny and checking compared to most mathematical proofs; but no matter how the process of verification plays out, it helps illustrate how mathematics evolves by rather organic psychological and social processes.

When people are doing mathematics, the flow of ideas and the social standard of validity is much more reliable than formal documents People are usually not very good in checking *formal correctness* of proofs, but they are quite good at detecting potential weaknesses or flaws in proofs.

To avoid misinterpretation, I'd like to emphasize two things I am *not* saying. First, I am *not* advocating any weakening of our community standard of proof; I am trying to describe how the process really works Careful proofs that will stand up to scrutiny are very important I think the process of proof on the whole works pretty well in the mathematical community. The kind of change I would advocate is that mathematicians take more care with their proofs, making them really clear and as simple as possible so that if any weakness is present it will be easy to detect. Second, I am *not* criticizing the mathematical study of formal proofs, nor am I criticizing people who put energy into making mathematical arguments more explicit and more formal These are both useful activities that shed new insights on mathematics.

I have spent a fair amount of effort during periods of my career exploring mathematical questions by computer. In view of that experience, I was astonished to see the statement of Jaffe and Quinn that mathematics is extremely slow and arduous, and that it is arguably the most disciplined of all human activities. The standard of correctness and completeness necessary to get a computer program to work at all is a couple of orders of magnitude higher than the mathematical community's standard of valid proofs. Nonetheless, large computer programs, even when they have been very carefully written and very carefully tested, always seem to have bugs.

I think that mathematics is one of the most intellectually gratifying of human activities. Because we have a high standard for clear and convincing thinking and because we place a high value on listening to and trying to understand each other, we don't engage in interminable arguments and endless redoing of our mathematics. We are prepared to be convinced by others. Intellectually, mathematics moves very quickly Entire mathematical landscapes change and change again in amazing ways during a single career.

When one considers how hard it is to write a computer program even approaching the intellectual scope of a good mathematical paper, and how much greater time and effort have to be put into it to make it "almost" formally correct, it is preposterous to claim that mathematics as we practice it is anywhere near formally correct.

Mathematics as we practice it is much more formally complete and precise than other sciences, but it is much less formally complete and precise for its content than computer programs. The difference has to do not just with the amount of effort: the kind of effort is qualitatively different. In large computer programs, a tremendous proportion of effort must be spent on myriad compatibility issues: making sure that all definitions are consistent, developing "good" data structures that have useful but not cumbersome generality, deciding on the "right" generality for functions, *etc* The proportion of energy spent on the working part of a large program, as distinguished from the bookkeeping part, is surprisingly small. Because of compatibility issues that almost inevitably escalate out of hand because the "right" definitions change as generality and functionality are added, computer programs usually need to be rewritten frequently, often from scratch.

A very similar kind of effort would have to go into mathematics to make it formally correct and complete. It is not that formal correctness is prohibitively difficult on a small scale—it's that there are many possible choices of formalization on small scales that translate to huge numbers of interdependent choices in the large. It is quite hard to make these choices compatible; to do so would certainly entail going back and rewriting from scratch all old mathematical papers whose results we depend on. It is also quite hard to come up with good technical choices for formal definitions that will be valid in the variety of ways that mathematicians want to use them and that will anticipate future extensions of mathematics. If we were to continue to cooperate, much of our time would be spent with international standards commissions to establish uniform definitions and resolve huge controversies.

Mathematicians can and do fill in gaps, correct errors, and supply more detail and more careful scholarship when they are called on or motivated to do so. Our system is quite good at producing reliable theorems that can be solidly backed up. It's just that the reliability does not primarily come from mathematicians formally checking formal arguments; it comes from mathematicians thinking carefully and critically about mathematical ideas.

On the most fundamental level, the foundations of mathematics are much shakier than the mathematics that we do. Most mathematicians adhere to foundational principles that are known to be polite fictions. For example, it is a theorem that there does not exist any way to ever actually construct or even define a well-ordering of the real numbers. There is considerable evidence (but no proof) that we can get away with these polite fictions without being caught out, but that doesn't make them right. Set theorists construct many alternate and mutually contradictory "mathematical universes" such that if one is consistent, the others are too. This leaves very little confidence that one or the other is the right choice or the natural choice. Gödel's incompleteness theorem implies that there can be no formal system that is consistent, yet powerful enough to serve as a basis for all of the mathematics that we do.

In contrast to humans, computers are good at performing formal processes. There are people working hard on the project of actually formalizing parts of mathematics by computer, with actual formally correct formal deductions. I think this is a very big but very worthwhile project, and I am confident that we will learn a lot from it. The process will help simplify and clarify mathematics. In not too many years, I expect that we will have interactive computer programs that can help people compile significant chunks of formally complete and correct mathematics (based on a few perhaps shaky but at least explicit assumptions), and that they will become part of the standard mathematician's working environment.

However, we should recognize that the humanly understandable and humanly checkable proofs that we actually do are what is most important to us, and that they are quite different from formal proofs. For the present, formal proofs are out of reach and mostly irrelevant: we have good human processes for checking mathematical validity.

5. What motivates people to do mathematics?

There is a real joy in doing mathematics, in learning ways of thinking that explain and organize and simplify. One can feel this joy discovering new mathematics, rediscovering old mathematics, learning a way of thinking from a person or text, or finding a new way to explain or to view an old mathematical structure.

This inner motivation might lead us to think that we do mathematics solely for its own sake. That's not true: the social setting is extremely important. We are inspired by other people, we seek appreciation by other people, and we like to help other people solve their mathematical problems. What we enjoy changes in response to other people. Social interaction occurs through face-to-face meetings. It also occurs through written and electronic correspondence, preprints, and journal articles. One effect of this highly social system of mathematics is the tendency of mathematicians to follow fads. For the purpose of producing new mathematical theorems this is probably not very efficient: we'd seem to be better off having mathematicians cover the intellectual field much more evenly. but most mathematicians don't like to be lonely, and they have trouble staying excited about a subject, even if they are personally making progress, unless they have colleagues who share their excitement.

In addition to our inner motivation and our informal social motivation for doing mathematics, we are driven by considerations of economics and status. Mathematicians, like other academics, do a lot of judging and being judged. Starting with grades, and continuing through letters of recommendation, hiring decisions, promotion decisions, referees reports, invitations to speak, prizes. ..we are involved in many ratings, in a fiercely competitive system.

Jaffe and Quinn analyze the motivation to do mathematics in terms of a common currency that many mathematicians believe in: credit for theorems.

I think that our strong communal emphasis on theorem-credits has a negative effect on mathematical progress. If what we are accomplishing is advancing human understanding of mathematics, then we would be much better off recognizing and valuing a far broader range of activity. The people who see the way to proving theorems are doing it in the context of a mathematical community; they are not doing it on their own. They depend on understanding of mathematics that they glean from other mathematicians. Once a theorem has been proven, the mathematical community depends on the social network to distribute the ideas to people who might use them further—the print medium is far too obscure and cumbersome.

Even if one takes the narrow view that what we are producing is theorems, the team is important. Soccer can serve as a metaphor. There might only be one or two goals during a soccer game, made by one or two persons. That does not mean that the efforts of all the others are wasted. We do not judge players on a soccer team only by whether they personally make a goal; we judge the team by its function as a team.

In mathematics, it often happens that a group of mathematicians advances with a certain collection of ideas. There are theorems in the path of these advances that will almost inevitably be proven by one person or another Sometimes the group of mathematicians can even anticipate what these theorems are likely to be. It is much harder to predict who will actually prove the theorem, although there are usually a few "point people" who are more likely to score. However, they are in a position to prove those theorems because of the collective efforts of the team The team has a further function, in absorbing and making use of the theorems once they are proven. Even if one person could prove all the theorems in the path single-handedly, they are wasted if nobody else learns them

There is an interesting phenomenon concerning the "point" people. It regularly happens that someone who was in the middle of a pack proves a theorem that receives wide recognition as being significant. Their status in the community—their pecking order—rises immediately and dramatically. When this happens, they usually become much more productive as a center of ideas and a source of theorems. Why? First, there is a large increase in self-esteem, and an accompanying increase in productivity. Second, when their status increases, people are more in the center of the network of ideas—others take them more seriously. Finally and perhaps most importantly, a mathematical breakthrough usually represents a new way of thinking, and effective ways of thinking can usually be applied in more than one situation

This phenomenon convinces me that the entire mathematical community would become much more productive if we open our eyes to the real values in what we are doing. Jaffe and Quinn propose a system of recognized roles divided into "speculation" and "proving" Such a division only perpetuates the myth that our progress is measured in units of standard theorems deduced This is a bit like the fallacy of the person who makes a printout of the first 10,000 primes. What we are producing is human understanding. We have many different ways to understand and many different processes that contribute to our understanding We will be more satisfied, more productive and happier if we recognize and focus on this.

6. Some personal experiences

Since this essay grew out of reflection on the misfit between my experiences and the description of Jaffe and Quinn's, I will discuss two personal experiences, including the one they alluded to

I feel some awkwardness in this, because I do have regrets about aspects of my career: if I were to do things over again with the benefit of my present insights about myself and about the process of mathematics, there is a lot that I would hope to do differently. I hope that by describing these experiences rather openly as I remember and understand them, I can help others understand the process better and learn in advance.

First I will discuss briefly the theory of foliations, which was my first subject, starting when I was a graduate student. (It doesn't matter here whether you know what foliations are.)

At that time, foliations had become a big center of attention among geometric topologists, dynamical systems people, and differential geometers. I fairly rapidly proved some dramatic theorems. I proved a classification theorem for foliations, giving a necessary and sufficient condition for a manifold to admit a foliation. I proved a number of other significant theorems. I wrote respectable papers and published at least the most important theorems. It was hard to find the time to write to keep up with what I could prove, and I built up a backlog.

An interesting phenomenon occurred. Within a couple of years, a dramatic evacuation of the field started to take place I heard from a number of mathematicians that they were giving or receiving advice not to go into foliations—they were saying that Thurston was cleaning it out. People told me (not as a complaint, but as a compliment) that I was killing the field. Graduate students stopped studying foliations, and fairly soon, I turned to other interests as well

I do not think that the evacuation occurred because the territory was intellectually exhausted—there were (and still are) many interesting questions that remain and that are probably approachable. Since those years, there have been interesting developments carried out by the few people who stayed in the field or who entered the field, and there have also been important developments in neighbouring areas that I think would have been much accelerated had mathematicians continued to pursue foliation theory vigorously.

Today, I think there are few mathematicians who understand anything approaching the state of the art foliations as it lived at that time, although there are some parts of the theory of foliations, including developments since that time, that are still thriving.

I believe that two ecological effects were much more important in putting a damper on the subject than any exhaustion of intellectual resources that occurred

First, the results I proved (as well as some important results of other people) were documented in a conventional, formidable mathematician's style. They depended heavily on readers who shared certain background and certain insights. The theory of foliations was a young, opportunistic subfield, and the background was not standardized. I did not hesitate to draw on any of the mathematics I had learned from others. The papers I wrote did not (and could not) spend much time explaining the background culture They documented top-level reasoning and conclusions that I often had achieved after much reflection and effort. I also threw out prize cryptic tidbits of insight, such as "the Godbillon-Vey invariant measures the helical wobble of a foliation", that remained mysterious to most mathematicians who read them This created a high entry barrier: I think many graduate students and mathematicians were discouraged that it was hard to learn and understand the proofs of key theorems.

Second is the issue of what is in it for other people in the subfield When I started working on foliations, I had the conception that what people wanted was to know the answers. I thought that what they sought was a collection of powerful proven theorems that might be applied to answer further mathematical questions But that's only one